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A
DISCOURSE
ON THE
Theory of Gunnery.

DELIVERED AT THE
Anniverſary Meeting of the ROYAL SOCIETY,
November 30, 1778.

K. Academies

By Sir JOHN PRINGLE Baronet.

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MDCCLXXVIII.

MVSEVM
BRITAN
NICVM

GENTLEMEN,

AMONG the ſeveral experiments communicated to the Society, during the courſe of the preceding year, none ſeeming ſo much to engage your attention, as thoſe contained in the Paper, intituled, *The force of fired gun-powder, and the initial velocity of cannon-balls, determined by experiments:* with much pleaſure therefore I acquaint you, that, on account of the pre-eminence of that communication, your Council have judged the author, Mr. CHARLES HUTTON, worthy of the honour of the annual medal, inſtituted on the beqeuſt of Sir GODFREY COPLEY Baronet, for raiſing a laudable emulation among men of genius, in making experimental inquiries. But, as on former occaſions, ſo now, your Council, waving their privilege of determining the choice, have acted only as a ſelect number deputed by you, to prepare matters for your final deciſion. I come then, on their part, briefly to lay before you the ſtate of the *Theory of Gunnery*, from its riſe to the time when its true foundation was laid, in order to evince how conducive thoſe experiments may be to this improvement of an art of public concern, as well as to the advancement of *natural knowledge*, the great object of your inſtitution. And if, upon a review of the ſubject, you ſhall entertain no leſs favourable an opinion of Mr. HUTTON's performance, than what your Council have done, it is their earneſt requeſt that you would enhance the value of this prize, by authorizing your Preſident to preſent it to our ingenious brother in your name.

ARTILLERY (in the large acceptation of the term) took place long before the invention of gunpowder. We trace the art to the remoteſt antiquity, ſince the Sacred Records acquaint us, that one of the kings of Judah, eight hundred years before the Chriſtian æra, erected on the towers and bulwarks of Jeruſalem engines of war, the contrivance of ingenious men, for ſhooting arrows and great ſtones for the defence of that city(a). Such machines were afterwards known to the Greeks and Romans by the names of *baliſta*, *catapulta* and others, which had amazing powers, and were not leſs terrible in their effects than the cannon and mortars of the moderns. It appears that the *baliſta* was contrived to ſhower volleys of darts and arrows of a very large ſize upon the enemy, whilſt the *catapulta* or *onagra* (as it was otherwiſe called) was fitted not only for that purpoſe, but for diſcharging ſtones of an enormous weight; I might ſay *rocks*, ſince ſome of them are reported to have weighed ſeveral hundred pounds. Batteries compoſed of numerous pieces of that kind of artillery, nothing could withſtand. Yet, if we are rightly informed, their ſole principle of motion conſiſted in the ſpring of a ſtrongly-twiſted cordage, made of animal ſubſtances ſingularly tough and elaſtic. Theſe warlike inſtruments continued, not only during the time of the Roman empire, but to the 12th and 13th centuries, as we find from hiſtory; nor indeed is it probable that they were totally laid aſide, till gun-powder and the modern ordnance, attaining a good degree of perfection, ſuperſeded their uſe. The very intelligent commentator of POLYBIUS(b) is of opinion, that the military art rather loſt than gained by the exchange of the *catapulta* for the mortar: but however that point may be determined in ſpeculation, it is not likely that the ancient *tormenta militaria* will ever be revived; but that all nations will keep to the art of gunnery and ſtudy how to improve it; that is, they will adhere to a ſyſtem of artillery, wherein the moving power depends on the expanſive force of gun-powder, or of ſome other ſubſtance of a ſimilar nature.

(a) 2 Chron. xxvi. 15.

(b) M. FOLARD.

Upon the firſt application of this principle to the purpoſes of war, nothing perhaps was leſs thought of than to aſſiſt ſo empirical a practice by ſcientific rules; for, however aiding in theſe matters the ancient mechanicians might have been, who, like ARCHIMEDES, had invented or perfected ſome of the *baliſtic* machines, no praiſe ſeemed now due to the mathematicians for either the diſcovery or improvement of the new artillery. In fact, we find the practice of the art had ſubſiſted about 200 years, before any geometer conſidered it as one that admitted a theory, or at leaſt ſuch a theory as was grounded on geometry.

It ſeems but juſt to trace and commemorate the inventors of the ingenious arts which furniſh matter for diſcourſes on theſe occaſions; and not only the main inventors, but even thoſe who firſt turned their thoughts upon the ſubject: for, though ſuch men may not have produced any thing perfect, yet they may have ſuggeſted ideas to others of a leſs inventive, but of a more executive genius, and who, unprovided with thoſe hints, would never have made any notable diſcovery. I muſt therefore obſerve, that the *Italians* were the firſt who emerged out of thoſe thick clouds of ignorance and barbariſm which had ſo long overſpread this quarter of the world. They profited by the unhappy fate of Conſtantinople; for by liberally receiving the learned emigrants on that diſtreſsful occaſion, they were largely repaid by their arts and ſciences, and ſtill more abundantly by their language, whereby they were enabled to read and to tranſlate thoſe ancient manuſcripts, which the Greeks had ſaved out of the wreck of their country. The art of printing, which was eſtabliſhed ſoon after, was the means of quickly diſſeminating thoſe treaſures of knowledge, and concurred with the fall of the eaſtern empire to form an epoch for the advancement of learning, unparalleled in the annals of letters.

The end of the 15th century, and the whole of the 16th, were chiefly employed by the Italians in the ſtudy and in the tranſlation of the old Greek authors. The geometry of the ancient Greeks, as well as the arithmetic in numbers and ſpecies of the Arabians, were cultivated; but both remained, as it were, ſciences by themſelves, unaſſiſting to, or at beſt but weak and reluctant auxiliaries to the philoſophy of the ſchools: and indeed how could the abſtracted doctrines of numbers and quantities be ſtrained to co-operate with a ſyſtem, in which neither the laws of motion, nor any but the ſuperficial, and often deluſive properties of matter, were to be met with? The genius of the Greeks, all acute and brilliant as it was, had never been properly directed to the interpretation of nature, and was indeed unfit (as Lord BACON pronounced) for a ſtudy that made ſo ſlow and painful a progreſs, by re-iterated and varied experiments and obſervations. It was no wonder then, if the *mixed mathematics*, as they are called, deſcended to the moderns in a ſtate no-wiſe correſponding to the elegance and certainty of thoſe parts of the ſcience which were elementary and pure; and that thoſe mixed parts ſhould have been found defective and erroneous, in proportion (if I may ſo expreſs myſelf) to the phyſical conſiderations that were to be taken into the inquiry. The imperfection of the ancients, with regard to natural philoſophy, was not perceived at that time; nay, at the period we are treating of, the learned were firmly perſuaded of the contrary, and that all that was wanting to be known concerning the laws of nature, and the properties of matter, was to be taken either directly, or by deduction, from the phyſics of ARISTOTLE. It was not till the 17th century was ſomewhat advanced, that men of ſcience began to liſten to Lord BACON and GALILEO, the great founders of the experimental and the true philoſophy.

Mean while, in the beginning of the 16th century, unqualified as the Italians then were for entering upon phyſico-mathematical inquiries (c), they nevertheleſs made the attempt, and in particular took the theory of projectiles into conſideration. Some imagined that a body impelled with violence, ſuch as a ball diſcharged from a cannon, moved in a right line till the force was ſpent, and that then it fell in another right line perpendicularly to the earth. Upon this principle, abſurd as it was, we find one of the earlieſt authors grounding his whole theory of gunnery[d]; whilſt others, diſſenting from his hypotheſis, admitted only the ſtraight line, in which the ball moved for ſome time after coming out of the piece, and that other ſtraight line in which it fell to the ground; but aſſerted that theſe two were connected by a curve line, and that this curve was the ſegment of a circle. NICOLAS TARTAGLIA of Breſcia, a mathematician of the firſt rank in thoſe days, and ſtill celebrated for his improvements in algebra, hath been ſuppoſed to be the author of this doctrine, no leſs erroneous than the former, and for which two of his books have been quoted[e]. Thoſe I have never ſeen; but from another of his works, profeſſedly written on this ſubject, and tranſlated into Engliſh under the title of *Colloquies concerning the art of ſhooting in great and ſmall pieces of artillery*[f], him I find, contrary to the opinion of his contemporaries, maintaining that no part of the track of a cannon-ball is in a right line, though the curvature in the firſt part of its flight be ſo ſmall, that it needeth not to be attended to. But TARTAGLIA is far from ſuppoſing, that the line in queſtion hath any relation to a *parabola*, or to any regular curve. It would

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(c) The chief exception that occurs to this general remark, is the rapid progreſs which in that age COPERNICUS made in aſtronomy; who was not indeed an Italian, but was ſuppoſed to have profited by his early travels into Italy, which he enlightened afterwards by his admirable diſcoveries.

(*d*) See MONTUCLA, Hiſt. des Mathem. vol. I. p. 623.

(*e*) Thoſe were *La Nuova Scientia*, and *Queſiti ed Inventioni diverſe*.

(*f*) Publiſhed at London, A. 1588.

would ſeem then, that if this mathematician had at firſt been ſo far miſtaken, as to fancy that ſome part of the courſe of a projectile was in a ſtraight line, he had afterwards changed his opinion, and was perhaps ſingular in what he finally embraced.

From numerous inſtances one would imagine, that in thoſe days, ſo far were men of ſcience from making experiments themſelves, that they even ſhut their eyes againſt what chance would have preſented to their ſight. For, whoever had minded the roving ſhot of an arrow, the flight of a ſtone from a ſling, or had attended to a ſtream of water iſſuing from the ſpout of a ciſtern, might have been convinced, that the path of every projectile was in a continued curve, whatever little he otherwiſe knew concerning the properties of that one.

But had the obſervation of the philoſophers gone ſo far, they had ſtill been at a diſtance from the truth. They might have perceived a likeneſs between the track of thoſe bodies in motion and a parabola, and concluded, from analogy, that all projectiles delineated that curve in the air; but they could never have realized their conjectures by mathematical demonſtration, without previouſly knowing the law of *acceleration* in falling bodies: a diſcovery reſerved for the next century, and for GALILEO[(g)], one of the greateſt ornaments of it.

It was he who firſt inveſtigated the effects of *gravity* on falling bodies, and upon that foundation demonſtrated, that all projectiles would move in a parabola in a non-reſiſting medium. And as he made little account of the reſiſtance of the air, whoſe properties were then imperfectly known, he proved that a ball ſhot horizontally would, in its flight, deſcribe half a parabola; and when the piece had an elevation above the horizon, the ball would deſcribe a whole parabola, ſuppoſing it to fall on the plane of the battery. By the ſame method of reaſoning he ſhewed, that whatever the ranges of the projected body, or the elevations of the piece were, the ball would ſtill trace that curve line, of a greater or leſſer amplitude, by the time it deſcended to the level of the place from whence it came.

Thus far went GALILEO, confining his projections to the horizontal plane of the battery; but TORRICELLI his diſciple ſoon after carried the theory farther, by tracing the ſhot to its fall, whether that place was above or below the plane; and ſtill found, by geometrical deductions, that it flew in a parabola of a larger or a ſmaller amplitude, according to the angle of elevation of the piece, and the ſtrength of the powder.

(g) He was born in the year 1564; but few if any of his works were publiſhed till after the year 1600, and his dialogues on motion not before 1638.

Various and numerous had been the diſputes in Italy about the laws of motion in general, and eſpecially about thoſe of projectiles, from the time the mathematicians had begun the inquiry, till the publication of the dialogues of GALILEO on that ſubject (a ſpace of upwards of a hundred years) but from that period, ſo evident did his demonſtrations appear, that all conteſt ceaſed, and every man of ſcience was convinced, that all projectiles moved in the track which he had diſcovered. For, as to the reſiſtance of the air, which he had not paſſed unnoticed (as GALILEO himſelf had been the firſt, at leaſt of the moderns, who ſtarted the notion of the weight of the air and the preſſure of the atmoſphere) yet ſo thin and ſo yielding did they eſteem that fluid to be, that they were aſſured it could occaſion no ſenſible, at leaſt no material, deviation from that curve. As they had the principle from GALILEO, ſo they believed themſelves warranted by that reſpectable author, not to fear from that cauſe any objection, which he himſelf had ſuggeſted, but had removed. *Among theſe projectiles* (ſays he) *which we make uſe of, if they are of a heavy matter and a round form*; *nay if they are of a lighter matter, and have a cylindrical form, ſuch as arrows ſhot from bows, their track or path will not ſenſibly decline from the curve of a parabola*[b].

Here then was the theory of gunnery laid, in appearance, on the moſt ſolid foundation. And thus far the Italians having proceeded, they ſeemed to have taken leave, and to commit the ſubject to other nations, whoſe greater power, or greater ambition, was more likely to make them avail themſelves of the perfection of a military art, than their inſtructors. We had reaſon therefore to expect, that a neighbouring ſtate, intent upon the advancement of the arts and ſciences in general, would not fail to give particular attention to thoſe that ſhould appear moſt ſubſervient to its grandeur. Accordingly we find, that our ſiſter-ſociety of that kingdom had not been many years eſtabliſhed, when an ingenious member of that illuſtrious Body, not

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(b) See his 4th Dialogue on Motion.

queſtioning the ſoundneſs of the Galilean principle in regard to projectiles, in the year 1677, propoſed to the academy, as a problem for the improvement of artillery, how to direct a piece (ſuppoſe a mortar) ſo as to make the ſhot fall where one had a mind; or in the common expreſſion, *to hit a mark*, the ſtrength of the powder being given(i). This thought met with general approbation, and ſo far were the academy from raiſing any difficulty about the obſtruction which the air might occaſion to a body moving with ſo much velocity in it, that we do not find the making experiments on that head was conſidered by them as an eſſential ſtep to the ſolution; but that their principal geometers ſtraightway ſet about ſolving the problem as it had been announced to them, ſome following one method, ſome another, and all upon the ſuppoſition of a projectile moving in the line of a parabola. But M. BLONDEL, who had been the propoſer, and who more particularly had ſtudied the queſtion, compoſed a large volume on the ſubject, which he publiſhed a few years after(k), under the title of *L'Art de jetter les Bombes*; a performance much celebrated at the time, and that continued in no ſmall requeſt long after, as containing, beſides his own, the labours of ſeveral other members of that ſociety of the moſt diſtinguiſhed merit. So many, and ſuch hands concurring in framing this work, it was no wonder that the learned throughout Europe were confirmed by it in the Galilean theory; and the more as M. BLONDEL had obviated the only objection they ſuppoſed could be made to it, the *reſiſtance of the air*, which he had taken care expreſsly to mention, and ſo to combate as to perſuade the reader, that the retardation arising from that cauſe was ſo inconſiderable as to be of no account in the practice.

(i) See Hiſt. de l'Academ. Roy. des Sciences, A. 1707.

(k) In the year 1683. See Hiſt. de l'Acad. R. des Sci. A. 1707.

This illuſion about the ſmall or non-reſiſtance of the air to bodies rapidly moving in it, was ſo prevalent at the end of the laſt century, and in the beginning of the preſent, that in the hiſtory of the Royal Academy for the year 1707, we find their worthy and moſt accompliſhed ſecretary, after taking notice of the joint labours of ſo many able mathematicians concerned in BLONDEL's publication, venturing to ſay, *it did not appear that any thing was then wanting for the practice of the art* [of Gunnery] *except perhaps perfecting the inſtruments for pointing a cannon or mortar . . . . . . but that geometry had*

had done its part, ſo to ſpeak, with regard to practice. &c.(l)

But far be it from our intention to relate the imperfections of others, in order to raiſe ourſelves by the compariſon. Candour requires of us not only to acknowledge, that in this country, as to the point in queſtion, we did not ſurpaſs our neighbours; but ingenuouſly to own that, on the contrary, we were perhaps more liable to exception. For, ſome years before Blondel's work appeared(m), a treatiſe was publiſhed by one of our own artilleriſts, Anderson (a perſon of eminence in his profeſſion) intituled *The genuine uſe and effects of the gun*, in which the author ſtrenuouſly ſupports the Galilean theory; nor do we learn he was ever contradicted among us, although he undertook to anſwer all thoſe who ſhould make objections to it. Nay, when he had an opportunity afterwards of making experiments on the ranges of bombs, and by thoſe trials was aſſured that their flight was not in a parabola; yet ſo far was he from aſcribing the deviation from that figure to the reſiſtance of the air, that he had recourſe to an hypotheſis,

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(l) Hiſt. de l'Acad. R. des Sc. A. 1707, under the article *Mechanique*.

(m) Viz. in 1674.

hypotheſis, repugnant to all the laws of motion, to ſalve appearances, and to reconcile thoſe experiments with his former doctrine(n).

And did not Dr. HALLEY, ſo long the ornament of this Society, communicate in the year 1686 a Paper, which he calls *A diſcourſe concerning gravity*, in which, treating of the motion of projectiles, he ſays, that being aware of the deflexion from the parabolic curve that might be occaſioned by the reſiſtance of the air, he had made ſome experiments, even with cannon-balls, to eſtimate the force of that reſiſtance; yet conclude, *That in large ſhot of metal, whoſe weight many thouſand times ſurpaſſed that of air, and whoſe force is very great, in proportion to the ſurface wherewith they preſs thereupon, this oppoſition was not diſcernible.* And again, *Though in ſmall and light ſhot, the oppoſition of the air ought and muſt be accounted for; yet in ſhooting great and weighty bombs, there need be very little allowance made; and ſo theſe rules* [thoſe, to wit, grounded on the principle of GALILEO] *may be put in practice to all intents and purpoſes, as if this impediment* [the reſiſtance of the air] *were abſolutely removed.*

(n) See his treatiſe *To hit a Mark*, publiſhed in 1690.

removed(o). Such concluſions, which we now find to be erroneous, were the leſs to be expected from ſo eminent a perſon, as they argued too much haſte to finiſh a theory, that was to be made ſubſervient to preſent uſe.

It might indeed have been expected, that men of ſcience applying themſelves to this ſtudy, would have been ſooner awakened to the conſideration of the great oppoſition of the air, by the *Principia* of NEWTON, publiſhed a little after this Paper of HALLEY'S(p). For in that excellent work the illuſtrious author had demonſtrated, that the curve deſcribed by a projectile, in a ſtrongly reſiſting medium, differed much from a parabola, and that the reſiſtance of the air was great enough to make the difference between the curve of projection of heavy bodies and a parabola far from being inſenſible, and therefore too conſiderable to be neglected.

Have we not then leſs to plead for not attending to the *Principia* of NEWTON in this article(q), than the mathematicians of other nations, who, as M. de FONTE-

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(o) Philoſ. Tranſ. N° 179, p. 20.

(p) In the year 1687.

(q) NEWTON, Princip. Mathem. lib. ii. ſect. 7.

NELLE obſerves(r), partly from the difficulty of underſtanding that conciſe and profound work, and partly from a miſapprehenſion of its tendency (which they fancied was to revive the exploded doctrine of *occult qualities)* were late in becoming acquainted with it? But it is not ſo eaſy to account for their inattention to HUYGENS, a known and even then a much eſteemed author, and who indeed was ſecond to NEWTON alone in ſcience and in genius. For he in the year 1690 had publiſhed a treatiſe on *Gravity*, written in a popular manner, wherein he gave an account of ſome experiments he had made at Paris, and in the academy, by which, as well as by mathematical inveſtigations, he was convinced of the truth of NEWTON's concluſions, in regard to the great oppoſition of the air to bodies moving ſwiftly in it; and, by conſequence, believed that the track of all projectiles was very different from the line of a parabola(s).

But excepting NEWTON and HUYGENS, the learned ſeemed univerſally to acquieſce in the juſtneſs and ſufficiency of the principles of gunnery invented by GALI-

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(r) Eloge de NEWTON.

(s) Diſcours de la Cauſe de la Peſanteur. Leide, 1690.

LEO, enlarged by TORRICELLI, confirmed and reduced to ſyſtem by ANDERSON, BLONDEL, HALLEY and others; and ſo far were the theoriſts, in that branch of ſcience, from ſuſpecting any defect or fallacy in theſe principles, that they ſeemed rather to reproach the practical artilleriſts, for not profiting more by the inſtructions which they had ſo liberally imparted to them. Nor do we find that an apology was made for the empirical exerciſe of the art, by any author of note in that line, earlier than the ſixteenth year of this century, when M. de RESSONS, a French officer of artillery, diſtinguiſhed by the number of ſieges at which he had ſerved, by his high military rank, and by his abilities in his profeſſion; when he, I ſay, thus qualified to bear teſtimony, preſented a *memoire* to the Royal Academy (of which he was a member) importing, that *although it was agreed that theory joined to practice did conſtitute the perfection of every art, yet experience had taught him, that theory was of very little ſervice in the uſe of mortars. That the work of M. BLONDEL had juſtly enough deſcribed the ſeveral parabolic lines, according to the different degrees of the elevation of the piece; but that practice had convinced him there was no theory in the effects of gun-powder: for that having endeavoured, with the greateſt preciſion,* *to point a mortar agreeably to thoſe calculations, he had never been able to eſtabliſh any ſolid foundation upon them* (t).

Thus, after the theory of gunnery had exerciſed the genius of the learned for nearly two hundred years, and for almoſt fourſcore of that time had reſted on fundamentals which had never been conteſted, it was pronounced at once to be almoſt intirely uſeleſs, and that by one of the moſt competent judges. Now, whether it were owing to the deference due to the authority of that experienced artilleriſt, or to ſome other cauſe, I ſhall not determine, but obſerve, that it appears not from the hiſtory of the academy, that the ſentiments of M. de Ressons were at this time controverted, or any reaſon offered afterwards for the failure of the theory of projectiles when applied to uſe. Nor can I paſs unnoticed the pauſe that enſued before any further attempts were made to improve the theory of the art, either upon the old principles or upon new ones, except by ſuch authors as ſeemed ignorant of this tranſaction, and who of courſe were not ſufficiently apprized of the inefficacy of the properties of the parabola for directing practice.

(t) Mem. de l'Acad. R. des Sc. A. 1716.

Or by thoſe who were employed in ſpeculatively inveſtigating the nature of the curve traced by a ball in the air; a curve which began at laſt to be conſidered as one deviating much from the line of a parabola. Or, finally, by ſuch as, having taken notice that NEWTON's ideas had not been duly attended to, endeavoured to avail themſelves of them, and of ſome experiments that had been made by others, for proving the great oppoſition of the air to bodies of ſwift motion; but without aſcertaining the degree of that reſiſtance, or enriching the art by any practical rules(u).

Such was the unhinged ſtate of this part of the mixed mathematics, when within our memory Mr. BENJAMIN ROBINS took cognizance of it: nor could the ſubject have fallen into abler hands, endowed as he was by nature with a ſuperior genius and unwearied application. Mr. ROBINS was deeply verſed in geometry and the doctrine of numbers; but he knew the limits as well as the powers of both, and how inſufficient they were for eſtabliſhing any theory where matter was concerned, without preparing the way, by finding out the phyſical properties of that *matter*, by many and varied experiments and attentive obſervation. Thoſe who had hitherto treated of the foundation of gunnery, by being too forward in the application of their mathematics, had in a manner hurt the credit of that admirable ſcience. They ought to have ſeen the neceſſity of minutely examining every circumſtance which could affect the courſe of a projectile, beſides that of gravity. Mr. ROBINS perceived the error of his predeceſſors in that inquiry, and corrected it. Perſuaded as he was from Sir ISAAC NEWTON'S *Principia* of the great reſiſtance of the air to bodies moving in it, and alſo of the uncertainty of the force of gun-powder, and of the variations in the flight of ſhot, occaſioned by the unavoidable varieties in the make of it, and in the make of the pieces of artillery which diſcharged it; apprized, I ſay, of ſo many cauſes of aberration, he juſtly concluded, that the foundation here was at leaſt as much an affair of phyſics as of geometry, and that if the art of throwing bombs had not been advanced by theory, it was not becauſe the art admitted of none, but becauſe the theory which had hitherto been deviſed had been both defective and erroneous. He ſuſpected that moſt of the writers on gunnery had been deceived, in ſuppoſing the reſiſtance of the air to be inconſiderable, and thence aſſerting the track of all ſhot to be nearly in the curve of a parabola, by which means it came to paſs that all their determinations, about the flight of projectiles of violent motion, had declined conſiderably from the truth. But in order to clear this point from every doubt, he found it neceſſary to aſcertain the force of gun-powder, and by that ſtep to eſtimate the velocity of the ſhot impelled by its exploſion. That being done, he proceeded to meaſure the quickneſs of a muſket-bullet, ſhot out of a given barrel, with a given quantity of powder; and to confirm the truth of his concluſions, he contrived a machine, by which the velocity of a bullet might be diminiſhed in any given *ratio*, by being made to ſtrike on a large body of a weight juſtly proportioned to it; whereby the ſwifteſt motions, which otherwiſe would eſcape our examination, were to be exactly determined by theſe ſlower motions that had a given relation to them. The machine was a large wooden pendulum, which ſwung freely, but in ſo ſlow a manner, that its vibrations could eaſily be counted, whatever was the celerity of the bullet diſcharged againſt it. The thought was ſimple, ingenious, and inconteſtably his own.

(u) DAN. BERNOULLI, Comment. Acad. Petropol. T. 2. & 3.

He next inquired into the reſiſtance made by the air to projectiles of rapid motion, and which he diſcovered to be much greater than had been ſuppoſed by any writer on the ſubject; and indeed ſo great, that it was manifeſt the curve deſcribed by any ſhot was very different from a parabola, and conſequently that all the applications of the properties of that conic ſection to gunnery were ſo erroneous as to be totally uſeleſs. For by means of this pendulum, placed at different diſtances from the mouth of the piece, he clearly demonſtrated how much a bullet, flying with a given velocity, would gradually loſe of that motion by the oppoſition of the air: therein furniſhing to the learned a ſignal and inſtructive inſtance of the fallacy of the moſt ſpecious theories, that do not proceed hand in hand with experiments.

I ſhould too much exceed the juſt bounds of a diſcourſe of this kind, were I to enter more minutely into the ſyſtem founded by Mr. ROBINS, confirmed and improved, as I find, by the labours of ſeveral of the learned in foreign parts of great celebrity(w). I ſhall only add, that his performance well deſerves the title he gives it of *The new principles of gunnery*, ſince the author may more properly be ſaid to have invented a new ſcience than to have added to an old one. And I believe I may venture to ſay, that no phyſico-mathematical diſquiſition hath done more honour to this country, or to the age, than the writings of Mr. ROBINS on this ſubject, which have been publiſhed, partly by this Society, partly by himſelf, and partly ſince his death (in the collection of his whole mathematical tracts) by his learned friend.

(w) It is alſo much to the honour of Mr. ROBINS, that his writings on this ſubject have been tranſlated into foreign languages by men that were the beſt judges of their merit. I need only name M M. EULER, and LE ROY.

But though our worthy brother will ever be celebrated for being the inventor of the true principles of gunnery, yet it would be too flattering to his memory, to ſay he had carried the theory of this art to perfection. He himſelf was far from entertaining ſo high an opinion of his labours; nay he expreſsly declared, that he left ſome material points to be inquired into at more leiſure (which other occupations and his immature death deprived him of) and he much regretted that he wanted conveniency and opportunities for making experiments on balls of a greater weight, than what he had uſed for aſcertaining the initial velocity of them.

Much therefore are we indebted to Mr. HUTTON, who, treading in the footſteps of the deceaſed, hath reſumed and proſecuted this laſt *deſideratum*, and hath ſhewn himſelf not unequal to ſo difficult an enterprize.

Mr. ROBINS, for determining the initial velocity of ſhot, ariſing from different quantities of powder, made uſe of balls of about an ounce weight; whereas Mr. HUTTON, for the ſame purpoſe, hath employed thoſe of different weights, from one pound to nearly three; or, in other words, Mr. ROBINS made trial with muſket-ſhot only, Mr. HUTTON with cannon-balls from 20 to about 50 times heavier. This was a conſiderable ſtep gained in a diſquiſition on that part of the ſcience, in which the reſiſtance of the air and other circumſtances were not concerned; and where neither analogy alone, nor mathematical deductions alone, nor the two combined, were ſufficient for eſtabliſhing principles applicable to the motion of cannon-balls, without making a new ſeries of experiments: and with what labour and judgment theſe have been performed, you underſtood by the account which Mr. HUTTON gave of them in his Paper.

But ſhould it now be inquired, what advantages may be derived from Mr. HUTTON's experiments, for the advancement of the art of gunnery, and of philoſophy in general? I would reply, that as to the former it may be ſufficient to obſerve, that though the improvements be only ſuch as can be deduced from the force of fired gun-powder; yet they are in a higher, more certain, and in a more general manner, than what reſulted from the labours of Mr. ROBINS; who indeed led the way, but who made, as it were in miniature, thoſe experiments which Mr. HUTTON hath executed at large, and which ROBINS himſelf wiſhed to have made, as well as others who have conſidered the ſubject ſince his time. Now theſe experiments, though made by Mr. HUTTON with cannon-balls of a ſmall ſize, may nevertheleſs form juſt concluſions when applied to cannon-ſhot of the largeſt ſize. And ſuch concluſions inform us of the real force of powder when fired, either in a cannon or a mortar, impelling a ball or bomb of a given weight; that is, they diſcover with what velocity a given quantity of powder drives thoſe projectiles in a ſecond, or in any other aſſigned portion of time. They alſo ſhew the law of variation in the velocity ariſing from different quantities of powder, with the ſame weight of metal, and likewiſe that law which takes place upon uſing balls of different weights. Further, they point out the advantage obtained by diminiſhing the windage in cannon, and teach us how we may increaſe the weight of the ſhot in the ſame piece, by making it of a cylindrical form, inſtead of a ſpherical: by this device, a ſmaller ſhip may be enabled to do the execution of a larger one. And experiments of the ſame kind will alſo determine the juſt length of cannon for ſhooting fartheſt with the ſame charge of powder.

Laſtly, it is from theſe experiments, or from others that may be made after the like manner, we are inſtructed how to anſwer every queſtion relative to military projectiles, except ſuch as depend on the reſiſtance of the air to bodies moving ſwiftly in it. This indeed is a conſideration which leaves room for greater improvement in the art, and for conferring freſh honours on thoſe, who, like Mr. Hutton, ſhall have opportunities and abilities for continuing and perfecting this very curious and uſeful inquiry.

As to the advantages accruing to philoſophy from the labours both of Mr. Robins and Mr. Hutton, ſpeak they not for themſelves? The ſciences of motion and pneumatics are promoted by them; and of what avail their perfection would be for the farther interpretation of nature, you need not be informed. In fine, we have here before us, in theſe experiments, the ſureſt teſt of our advancement in true knowledge, which is, the improvement of a liberal art, and the enlargement of the powers of man over the works of creation.

Some however may think, that the objects of this Society are the arts of peace alone, not thoſe of war, and that conſidering how numerous and how keen the inſtruments of death already are, it would better become us to diſcourage than to countenance their farther improvement. Theſe naturally will be the firſt thoughts of the beſt diſpoſed minds. But when upon a cloſer examination we find, that ſince the invention of arms of the quickeſt execution, neither battles nor ſieges have been more frequent nor more deſtructive, indeed apparently otherwiſe; may we not thence infer, that ſuch means as have been employed to ſharpen the ſword, have tended more to diminiſh than to increaſe the number of its victims, by ſhortening conteſts and making them more deciſive. I ſhall not however inſiſt on

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maintaining ſo great a paradox; but only ſurmiſe, that whatever State would adopt the Utopian maxims, and proſcribe the ſtudy of arms, would ſoon, I fear, become a prey to thoſe who beſt knew how to uſe them. For yet, alas! far ſeem we to be removed from thoſe promiſed times, *when nation ſhall not lift up ſword againſt nation, neither ſhall they learn war any more!*

Here ended the Preſident's Diſcourſe: after which he turned to Mr. HUTTON, *and ſaid,*

YOU have heard, Sir, the account I have given of the riſe and progreſs of the *theory of gunnery*, and of your improvement of it; a recital, which by no means would have done either you or the ſubject juſtice, had it been addreſſed to any other audience than to the preſent. But as my intention was only briefly to recall to the memory of theſe gentlemen what they knew of this ſubject, antecedently to your Paper, and to remind them of the reſult of your experiments, I flatter myſelf I have ſaid what was ſufficient on the occaſion: being now authorized by them to deliver into your hand this medal, as the perpetual memorial of their approbation. And let me add, Sir, that they make you this preſent with the more cordial affection, as by your other ingenious and valuable communications they are aſſured, not only of your talents, but of your zeal, for promoting the intereſts and honour of their Inſtitution.

MVSEVM
BRITAN
NICVM